chemical actions. The most familiar of such bodies are ptyalin, pepsin, pancreatin, of animal origin, with diastase, emulsin, and myrosin, as products of certain vegetable tissues. And when we learn that the actions brought about by these bodies are also nearly all of them capable of being induced by mere acids and alkalies acting under known conditions, much of that air of mystery seems stript from fermentative processes which Pasteur's definitions are calculated to inspire*.

The products of the animal and the vegetal organism are many of them capable of being built up by synthetic processes in our laboratories. No one now will venture to say that though such or such bodies can be artificially engendered, certain other organic compounds cannot, and never will be, so produced; and the same may be said in regard to actions supposed only to be producible by the direct agency of living units. As M. Schutzenberger says, in the preface to his work on 'Fermentation':—"If living cells produce reactions which seem peculiar to themselves, it is because they realize conditions of molecular mechanism which we have not hitherto succeeded in tracing, but which we shall, without doubt, be able to discover at some future time. Science can gain nothing by being limited in the possibility of the aims which she proposes to herself, or the end which she seeks."

But we must now turn to another side of the question, and see how, with increasing knowledge and new surmises concerning the death-point and life-history of ferment-organisms, possibilities of a new order in regard to their survival under adverse circumstances have been opened up. We must try also to estimate their relative worth.

In the year 1871† I made the first recorded experiments to ascertain at what precise temperature below 212° F., Bacteria, Torulæ, and their germs were killed. These experiments were conducted by adding a drop or two of a fluid swarming with such organisms to an artificially prepared nourishing solution (having a *neutral* reaction), in which they had been found to multiply rapidly, and then after exposing this inoculated mixture to definite temperatures for a certain time, putting it aside under favourable conditions to see whether it would or would not become

* Schutzenberger, 'On Fermentation' (English translation), in International Scientific Series, 1876, pp. 269–307.

† 'Modes of Origin of Lowest Organisms,' 1871, p. 50.

turbid*. Acting in this way I found that fluids heated to 131° F. (55° C.) for ten minutes would ferment, but that when heated to 140° F. (60° C.) for the same period they remained quite clear and pure. I inferred therefore that the temperature of 131° F. was not sufficient to kill, but that one of 140° F. was adequate to destroy the organisms and germs introduced into the nourishing liquid. I also ascertained that when the exposure to heat was prolonged to four hours such organisms were killed at still lower temperatures†.

In the following year Prof. Cohn and Dr. Horwath made almost similar experiments in regard to the death-point of Bacteria, but not for Torulæ. It does not appear that they were then aware of my investigations. They, however, arrived at results almost precisely similar, as may be seen when Prof. Cohn‡ says:—"These experiments demonstrated without exception that no Bacteria were developed in the flasks which were kept at a temperature of from 60°–62° C. for an hour, and that the contained fluid remained clear; on the other hand, flasks containing Bacteria fluid which had only been heated to 50° C. or 40° C. became clouded, in consequence of the multiplication of Bacteria, in a time varying from two to three days."

"One need scarcely mention," he adds, "that in flasks heated to 70°, 80°, 90° C. there was never any cloudiness. The fact that the cloudiness in a flask which has only been subjected to a temperature of 50°–52° C. for one hour shows itself much sooner than in one which has been kept at the same temperature for two hours, leads one to suppose that 60° C. is apparently not the lowest temperature at which Bacteria are killed, but that perhaps a lower degree of heat would be sufficient to prevent their multiplication."

In the year 1873 I returned to this subject in order to ascertain whether Bacteria and Vibriones would be killed at the same temperatures in organic infusions as they had been found to be in a neutral saline solution. A large number of experiments were

* The nourishing liquid employed was much simpler than that of Pasteur or of Cohn, since it consisted merely of a 2 per cent. solution in distilled water of the neutral ammonium tartrate with about $\frac{1}{2}$ per cent. of a neutral sodic phosphate. I had ascertained even in 1870 ('Nature,' July 14, p. 222) that both Bacteria and Torulæ would grow and multiply in a simple solution of ammonium tartrate, and therefore had recognized before Prof. Cohn that these organisms could take their carbon from such a compound as tartaric acid.

† 'Modes of Origin of Lowest Organisms,' 1871, p. 59.

‡ Beiträge zur Biologie der Pflanzen, 1er Bd., 2tes Heft, 1872, p. 219.

therefore made with *neutral* hay-infusion and with *acid* turnip-infusion inoculated with drops of a fluid swarming with the above-named organisms. The mixture was exposed to the several degrees of heat only for a period of *five* minutes. In a communication to the Royal Society, after classifying the results, I say* :—"The experimental results above tabulated seem naturally divisible into three groups. Thus, when heated only to 131° F., all the infusions became turbid within two days, just as the inoculated saline solution had done. Heated to 158° F., all the inoculated organic infusions remained clear, as had been the case with the saline solutions in my previous experiments when heated to 140° F. There remains, therefore, an intermediate heat-zone (ranging from a little below 140° F. to a little below 158° F.) after an exposure to which the inoculated organic infusions are apt to become more slowly turbid, although inoculated saline solutions raised to the same temperatures invariably remained unaltered." The cause of these discrepancies was further studied; and in a subsequent communication to the Royal Society a few months later, I adduced evidence to show that the turbidity which had occurred after the organic inoculated fluids had been subjected to a heat of 140° F. (60° C.) and upwards to 158° F. (70° C.) had been due "not to the survival of the living units, but rather to the fact that the mere dead organic matter of the *inoculating compound* has acted upon the more unstable organic infusions in a way which it was not able to do upon the boiled saline fluids"†. The inoculating compound made use of in these experiments being a fluid in full fermentation, it would contain, besides organisms and their germs, mere organic matter, and, as the light of subsequent investigations enables me to add, possibly some soluble chemical ferments produced during the vital activities of the living organisms themselves.

These experiments were devised and carried out solely with the view of throwing light upon one particular question, viz. the thermal death-point of Bacteria and their germs when immersed in fluids. Those of the first series also had the same reference to Torulæ and their germs. Having such an object before me I was careful to eliminate any source of confusion which might arise from the possible germinality of the mere fluids with which expe-

* Proceedings of the Royal Society, vol. xxi. p. 231.

† Ibid. pp. 325–330.

riment was made. I ascertained first of all, therefore, that the fluids destined to be employed under certain definite conditions as nourishing fluids were capable of acting efficiently as such, and that under those particular conditions they never of themselves behaved in such a manner as to make it possible to think that a *de novo* production of living matter would occur therein. This source of doubt being eliminated, one could watch the effects of inoculating such fluids with living Bacteria and of subsequently heating the mixture to different degrees, and draw tolerably safe conclusions therefrom. Without such a precaution it is obvious that, in the present state of this question, great mistakes might be made, since effects possibly due to the germinality of the fluids as such might be attributed to a supposed survival of the germs which had been heated in the inoculated fluids. I feel by no means sure that some of the recent investigators working in Prof. Cohn's laboratory have been quite so mindful of this point as they should have been.

I believe my experiments to have shown that a temperature of 140° F. (60° C.) is destructive to Bacteria, Vibriones, Torulæ, and their germs in a neutral saline solution, and that the same temperature is also destructive to Bacteria, Vibriones, and their germs both in a *neutral* hay-infusion and in an acid turnip-infusion. Even if we allowed the opposite interpretation to stand in regard to those cases with the organic infusions in which there was some room for doubt, we should still have to raise the death-point only to 158° F. (70° C.), and this, too, when the exposure to such a temperature had only been prolonged for five minutes.

Not the least countenance was given to M. Pasteur's notion that Bacteria- and Vibrio-germs could resist a higher temperature in neutral than they could in acid fluids. If there was any difference as between neutral hay- and acid turnip-infusion, it seemed slightly in the other direction.

These experiments were supposed to hold good, as I pointed out at the time, for "germs" as well as for the parent organisms*. The nourishing media were inoculated with a fluid in which Bacteria and Vibriones were multiplying rapidly, so that we had a right to infer that they were multiplying in their accustomed manner. I then said, "These experiments seem to show, therefore, that even if Bacteria do multiply by means of invisible gemmules, as well as by the known process of fission, such invisible

* Proceed. of Royal Soc. vol. xxi. p. 227.

particles possess no higher power of resisting the destructive influence of heat than the parent Bacteria themselves possess."

In 1875 Professor Tyndall began to work at this subject and announced his results early in the following year*. He did not endeavour to ascertain the lowest temperature which would prove destructive to Bacteria, Torulæ, and their germs, though he came to the conclusion that they were always killed by being boiled for five minutes in organic fluids, and he seemed to imply that this result was irrespective of the precise degree of acidity or neutrality of the fluids employed†. Since this conclusion as to the death of ferment organisms and their germs in infusions raised for a few minutes to 212° F. was based upon about five hundred experiments with fluids of the most varied nature, Prof. Tyndall seemed to feel considerable confidence in its truth. So far as it went, therefore, his evidence on this part of the subject was entirely confirmatory of mine. Indeed, in the beginning of 1876, Professor Tyndall's views on this important subject were as much opposed to those of M. Pasteur as mine were; we both disbelieved, as we thought, on good evidence, in the survival of germs in boiling neutral or faintly alkaline fluids.

At this time M. Pasteur's positive results with some of such fluids would seem to have been forgotten by Prof. Tyndall. At all events, not being able himself to get evidence that any boiled and guarded fluids would ferment, he attempted to throw discredit upon me because I had obtained such results. Forgetful of Pasteur's experiments above referred to, and apparently unaware of the confirmation which my experimental facts had obtained at the hands of many independent workers, he triumphantly brought forward a "cloud of witnesses" to convince the Royal Society and the world of science generally, as well as others, that my particular results in which fermentation had been made to show itself in boiled and guarded fluids were due to experimental errors into which it was conjectured that I had easily fallen, since it required all Prof. Tyndall's great skill and long experience to avoid them. He strenuously denied that a certain experimental result could be obtained when strict methods were followed. It was as regards the question of fact, rather than in regard to its interpretation, that Prof. Tyndall then did his best to throw discredit upon my work.

* Philosophical Transactions, 1876, pt. i. p. 27.

† *Loc. cit.* p. 51.

All this confident assertion and conjecture on the part of the new worker was based upon his belief, and is to be taken as the measure of his certainty at that time, that Bacteria and similar organisms, with their germs, were killed by being heated in fluids to 212° F. for a minute or two. It is, in truth, even now almost impossible otherwise to account for the continued barrenness of his 500 various fluids, placed, as he says, under conditions favourable for the multiplication of any organisms or germs which they might contain, not for days only, but for weeks and even months.

Professor Tyndall seems entirely to have misconceived the real aspect of the question as it stood before the scientific world in the beginning of 1876. He unhesitatingly coincided with me as regards the only point which was really in dispute, viz. whether the "omnipresent" ferment-organisms and their germs were killed by a brief boiling of them or not; whilst the fact which he called in question was the very point which had been abundantly confirmed and was then generally admitted, whatever interpretation might have been put upon it by different experimenters*. Indeed, what Prof. Tyndall had been unable to achieve in the way of inducing fermentation in boiled and guarded fluids, had three years previously been brought about by me in the presence of a highly skilled and then sceptical witness, Professor Burdon Sanderson. He subsequently published his declaration† that positive results, both with acid and with neutral boiled infusions, had been obtained without experimental flaw; yet in spite of this testimony, and without even mentioning it, Prof. Tyndall sought to decry my experiments and set aside my results.

Meanwhile, almost at the time that the learned physicist was acting in this bewildering manner, one of the principal authorities on such subjects in Europe, Prof. Ferdinand Cohn, was again confirming my impugned experiments, at Breslau, and was obtaining, both with acid and with neutral boiled infusions, those evidences of fermentation which hitherto Professor Tyndall had strangely enough failed to reproduce‡. The fact was again fully admitted by Prof. Cohn, though my interpretation of it was still questioned. It is therefore quite needless for me here even to

* For a list of such experimenters see 'Nature' Feb. 10, 1876, p. 284.

† 'Nature,' Jan. 8th, 1873.

‡ 'Beiträge zur Biologie der Pflanzen,' 1876, p. 259. This confirmation, after Prof. Tyndall's denial, was very similar in its opportuneness to that of Prof. Sanderson after E. Ray Lankester's denial (Quart. Journ. of Microsc. Science, Jan. 1873, vol. xiii. p. 74).

cite the other investigators who had previously obtained similar results. This side of the question has, in fact, been so thoroughly settled by my experiments, and the numerous confirmations which they have received at the hands of others, that it would be waste of space for me now to dwell further upon this part of the subject.

It must be obvious that what we need at present is all the definite evidence that can be obtained as to the thermal death-point, and as to the powers of resistance under different conditions, of ferment-organisms and their germs. Thus it is that I now restrict my remarks almost wholly to investigations bearing directly or indirectly upon this section of the subject.

Twelve months later, we find Prof. Tyndall* announcing that he was then able to obtain the previously denied results. The behaviour of his recent infusions had completely stultified his previous position. He was no longer at issue with me and others in regard to the fact. The difference between us was now one of interpretation only. In spite of his previously much-vaunted 500 negative results, and the good evidence which they supplied as to the death-point of Bacteria and their germs, Prof. Tyndall now endeavoured, as best as he could, to cover his previous unfortunate position. The result was a complete change of front.

During all his earlier experiments, though operating in the midst of London in an air which he had himself not lightly stigmatized, in many trials with all sorts of fluids, he had not come across a single germ which could survive the influence of boiling water for a few minutes. Desiccation of germs, according to Prof. Tyndall's experience at this time, would seem to have been a phenomenon of the rarest occurrence; germs capable of resisting a short boiling must have been almost, if not quite, unknown.

But no magician with his wand ever wrought a more complete change than did Prof. Tyndall by introducing a bundle of "old hay" into his laboratory. Henceforth there was evidence of fermentation in boiled fluids without stint, desiccated germs were everywhere—germs capable of resisting even two, three, four, and more hours of boiling everywhere surrounded him and got into his infusions.

These, at least, are the hypotheses by which Prof. Tyndall endeavoured to reconcile his earlier with his later results. But two things strike one as very unsatisfactory in regard to them and

* Brit. Med. Journ., Jan. 27, 1877, p. 95.

his method of supporting their cogency. In the first place, it may be observed that the fact of his having introduced a bundle of "old hay" into the laboratory of the Royal Institution cannot be regarded as a satisfactory explanation of the results of myself and others who had been able to obtain fermentation in boiled fluids long before, without the aid of any such magician's wand as this which Professor Tyndall had chanced to employ. Secondly, there is the very dubious nature of the evidence by which he has sought to support his interpretation, and the absence of any thing in what he has yet published on the subject which gives any definite or independent foundation to his new hypotheses. Thus, to take one illustration, in the Proceedings of the Royal Society* there is printed a note "On Heat as a Germicide when Discontinuously Applied," in which Prof. Tyndall says:—"Following up the plain suggestions of the germ-theory, I have been able, even in the midst of a virulently infective atmosphere, to sterilize all the infusions by a temperature lower than that of boiling water. * * * Before the latent period of any of the germs has been completed (say a few hours after the preparation of the infusion), I subject it for a brief interval to a temperature which may be under that of boiling water. Such softened and vivified germs as are on the point of passing into active life are thereby killed; others not yet softened remain intact. I repeat this process well within the interval necessary for the most advanced of those others to finish their period of latency. The number of undestroyed germs is further diminished by this second heating. *After a number of repetitions, which varies with the character of the germs, the infusion, however obstinate, is completely sterilized.*"

Noting by the way that the "character of the germs" has no other reality than Prof. Tyndall chooses to infer from the obstinacy of the infusion in resisting sterilization, it is only necessary further to point out that the above procedure and its results allows absolutely no conclusion to be drawn in favour of the survival of germs, except by ignoring the only other legitimate interpretation. The frequent repetitions of destructive heating might, after a time, repress all tendency to fermentative change in a fluid with the same facility that it might destroy germs supposed to be successively awakening to life and activity. If an investigator has decided beforehand that one of these possibilities is not worth

* No. 178, vol. xxv. p. 569.

thinking of as an interpretation, the problem, to his mind, is, of course, a simple one.

It is true, however, that the course of events between the period of the publication of Prof. Tyndall's first and that of his second paper did give to his explanation of these second results some semblance of support—although they were, at the same time, in rather flagrant contradiction with the uniformly negative results of his first five hundred trials.

The new point of view introduced in the mean time through the labours of Professor Cohn, in conjunction with those of Dr. Eidam and Dr. Koch, was thus brought about.

Dr. Eidam* carried out some researches in 1875 under Prof. Cohn's direction as to the exact death-point of *Bacterium termo*. He ascertained that this organic form always disappeared in fluids heated to a temperature of 113° F. (45° C.), though Bacilli were found growing and multiplying rapidly therein. In the following year these researches were continued in regard to Bacilli, and the results are given by Prof. Cohn† in his own memoir on these organisms, which was published in the autumn of 1876. The points of most significance therein recorded are (*a*) that Bacilli are the organisms which commonly make their appearance when previously boiled fluids undergo fermentation; (*b*) that at a temperature of 37° C., or thereabouts, when the infusions are exposed to air through a cotton-wool plug, these Bacilli grow into threads which accumulate in the form of a pellicle on the surface; (*c*) that in twenty-four to forty-eight hours a number of highly refractive particles appear at short distances from one another within the threads, which are to be regarded as "spores"; and (*d*) there is a certain amount of evidence, but not of a conclusive character, to show that these "spores" in a dry condition may resist heat much better and for a longer time than their parent organisms. This latter evidence is inconclusive, principally because no sufficient precautions were taken to show that what was attributed to survival of germs might not have been really due to a still-continuing germinality of the fluids, though also in part because the possible action of mere chemical ferments was not duly considered.

These points were reenforced in the same number of Prof.

* Beiträge zur Biologie der Pflanzen, 1er Bd. 2tes Heft, p. 208.

† Ibid. 2er Bd. 2tes Heft, p. 268.

Cohn's journal in an important paper by Dr. Koch entitled "Die Aetiologie der Miltzbrandkrankheit." The organism met with in this disease is also a Bacillus, indistinguishable by the microscope from that found in hay-infusions. When exposed to air at a temperature of about 37° C. it also grows into filaments, which speedily develop therein the highly refractive spore-like bodies, and then become disintegrated. Koch found, moreover, that the Bacilli themselves of splenic fever could only resist a comparatively short amount of desiccation, though he concluded from his experiments that the spores could retain their vitality and power of communicating the disease for years when imbedded in the midst of certain dried matter. No sufficient details, however, are given in regard to this latter point; and it cannot be considered that Koch's evidence proves that Bacilli spores can resist prolonged periods of desiccation, (1) because he found that the splenic-fever matter, containing germs, was only potent when pieces of spleen or blood in mass were dried, in *the midst of which the germs may not have been really desiccated at all*; and (2) because it has not yet been distinctly proved that it is the actual spores, or only the spores and not certain chemical principles in the medium, constituting soluble or 'particulate' ferments, which communicate the disease. *Any such chemical principles might preserve their integrity in the midst of the colloid masses above mentioned just as well as spores.*

This latter consideration is especially strengthened by recent accessions to our knowledge. Thus we learn from Dr. Koch himself that though the hay-Bacillus is so similar to the Bacillus of splenic fever as to be microscopically indistinguishable therefrom, yet that the former organisms are quite powerless to excite splenic fever when inserted beneath the skin of rabbits. And although it may be said that morphological similarity does not necessarily imply identity in the physiological or molecular actions of the two organisms, yet it may fairly be insisted that, as regards these two organisms in particular, there is evidence that in all outward respects their course of life and properties are also similar. But even greater need for caution in the same direction might be brought home to us by the now admitted fact that the common septic ferment excretes or helps to form a chemical principle *, existing, according to Prof. Burdon Sanderson, in the form of minute

* Just as other allied organisms give rise to grains of blue or other pigment in the jelly which envelops them.

particles capable of generating a febrile illness resembling septicæmia, which the organism itself is unable to produce—and also from the fact discovered by Musculus, and confirmed by Pasteur, that a soluble ferment exists in fermenting urine, separable therefrom, and which is capable of producing precisely the same changes in this fluid as may be initiated by the living ferment itself.

Yet, relying principally upon this evidence of Koch, Prof. Cohn postulates for the spores of hay-Bacilli a power of resisting prolonged and thorough desiccation. And although Koch says distinctly that he found no evidence of a survival of power to communicate the disease when he dealt with small fragments of splenic-fever matter which had been dried, Cohn assumes that for hay-Bacillus even separate spores, or spores in association with very small particles of matter, may preserve their potency; nay, further, that the conduction of heat no longer takes its ordinary course in regard to such particles—so that they may remain for hours immersed in fluids at temperatures destructive to all other visible forms of living matter. Before all these difficulties I may perhaps be pardoned for saying that I am not ready to yield assent to the popular view. Mere surmises and guesses must make way for definite knowledge acquired by accurate and crucial experimentation. But as yet there is nothing of this sort to support the notion of the ability of the ferment-organisms to endure complete desiccation, and that in this state they are able to resist for a prolonged period the otherwise destructive influence of heated fluids.

A fatal lack of precision seems to have pervaded all attempts which have yet been made to deal with the question of the ability of organisms or their germs to resist desiccation. This lack of precision is seemingly due to the fact that the mind of the experimenter is generally to a great extent biased by the notion of the impossibility of a generation *de novo* of living matter. Just as we have previously seen Pasteur inoculating barren fluids with organic débris &c. filtered from the air, and assuming that the fertility which ensues *must* have been due to this matter containing living germs, so, more recently, we find other experimenters subjecting such matter and the organisms which it may contain, or subjecting organisms and portions of the media in which they have been flourishing, to desiccating influences, and invariably attributing any supervening fermentation or disease which such matter may set up to the survival of the organisms, when the above-named results may have been due to the survival of mere chemical ferments or 'particles' in such desiccated media. This objection I pointed out in 1872 *, and it is one which must be met before conclusive experiments can be made. Fortunately the means for complying with this necessity are now within the reach of all skilled experimenters†. This kind of differentiation requires to be made especially by those who announce positive results. It would be a matter of less urgency wherever accurate experiments show an inability to resist desiccation, or to resist this process *plus* the brief influence of boiling water.

Before referring to a few inquiries which I have myself made in these new directions, it seems desirable to say a few words concerning one other attempt to raise the standard of vital resistance to heat for the germs of some organisms, as this particular evidence has been frequently mentioned during the last year—in fact, ever since Prof. Tyndall's contradictory experiments had in some way to be explained. Nothing better shows the paucity of any thing like exact knowledge concerning the ability of living matter to withstand a temperature of 212° F. and upwards, together with the strongly felt desire of the panspermatists to find it, than the altogether undue importance which has of late been attached to this evidence, which was brought forward nearly four years ago by the Rev. Mr. Dallinger and Dr. Drysdale.

These gentlemen are now well known as the authors of some very elaborate and meritorious investigations on the life-history of certain flagellate monads. In addition to reproduction by the well-known process of multiple fission, they have described two kinds of germs, one minute, but easily visible, and the other so minute as to be quite indistinguishable individually, even by the highest powers of the microscope. Some observations have been made as to the effects of different temperatures upon the parent forms and upon these reproductive units. The manner in which this investigation was conducted is thus described by the authors ‡:—"An ordinary slide containing adult forms and sporules covered in the ordinary way was allowed to evaporate slowly in

* 'The Beginnings of Life,' vol. ii. p. 4.

† Brit. Med. Journ., Feb. 13, 1875, p. 201.

‡ Monthly Microsc. Journ., Aug. 1873, p. 57.

seven instances, and placed in a dry heat which was raised to 121° C. It was then slowly cooled and distilled water allowed to insert itself by capillary attraction. On examination, all the adult forms were absolutely destroyed, and no spore could be definitely identified. But after having been kept moist in the growing stage for some hours, and watched with the $\frac{1}{50}$, gelatinous points were seen in *two out of the seven cases*, which were recognized as exactly like an early stage of the developing sporule, which were watched till they had reached the small flagellate state shown in fig. 5, pl. xxvi. The remaining five were barren of result." Other observations were detailed in a subsequent number of the same Journal *; but that above quoted is typical as regards the method, aud not far from typical as regards the results, which may fairly be described as eminently contradictory in nature. All the observations which these experimenters record I have tabulated, so that it may be seen what their evidence really amounts to :—

Nature of Heat-exposure.	*Survival of Spores.*	*Survival of Sporules.*
93°·33 C. (200° F.) for ten minutes.	On 3 slides out of 6.	On 3 slides out of 6.
"Raised to 121° C."	On 2 slides out of 7.	No observations made.
121°·11 C. (250° F.) for ten minutes.	Statement of results not precise, uncertain whether both *germs* and *sporules* developed in only 1 or in 4 out of 5 slides.	
"Heated up to 148°·88 C." (300° F.).	On 2 slides out of 6.	On 3 slides out of 6.

The authors say that these " are only typical results of a larger series of experiments." They are perhaps more typical than significant: they are assuredly very perplexing. Why, with such apparent uniformity of conditions, should there be so much discord in results ? These remarkable *sporules* would seem to be better able to withstand a momentary exposure to a temperature of 148° C. than one of the same duration to 121° C., and just as well able to bear this heat as an exposure for ten minutes to the very different temperature of 93°·30 C.

But why should the authors have deliberately thrown an ele-

* March 1874, p. 99.

ment of confusion into the subject which was wholly needless and easily to have been avoided?

The effects of dry heat (which is well known to be less damaging to life) are set forth when it would have been quite as easy and much more satisfactory to have given instead, or, in addition, the effects of heat upon these organisms and their germs when they were in the moist state *. The consequence has been that these results with dry heat have been quoted by subsequent writers as though they fell into the same category as others which have been made with moist heat; and differences of result which, in the main, have been due to different modes of exposure, have been ascribed to different powers of resistance on the part of simple organisms and their germs. Professor Tyndall, indeed, has gone so far as to speak of the "grave error" which biologists have hitherto made in failing to recognize this important distinction concerning germs and organisms respectively; yet, as a matter of fact, the possibility of such a difference has been clearly before the minds of all the principal workers on these subjects from the time of Spallanzani downwards †.

But the fallacy of all this may be seen from the fact that in 1862 M. Pasteur ‡ himself found that certain of the germs or spores of fungi, especially those of the common mould *Penicillium*, would germinate after exposure to a dry heat of 121° C. (250° F.) for thirty minutes, though, as he says in an earlier part of his memoir (p. 60), he had proved by direct experiment that when immersed in fluid, even for a few minutes, at a temperature of 100° C., all such germs were killed. Seeing that, according to the experience of Dallinger and Drysdale, their spores and sporules, in the dry state, were no better able to resist the momentary influence of 121° C. than of 148° C., and that Pasteur found spores which could resist a dry heat of 121° C. even for *thirty minutes* were yet invariably killed when immersed in boiling water for two or three minutes, there is no reasonable ground whatever

* It would have been perfectly easy to have put one or two drops of the fluid into a small tube, to have hermetically sealed it, and then to have heated it for 10 minutes or more to different degrees before subjecting the fluid to a prolonged microscopical examination upon a carefully prepared slide.

† In my 'Evolution and the Origin of Life,' 1874, pp. 141–168, Spallanzani's views on the subject are set forth and duly considered, as well as all other evidence that was at the time available.

‡ Ann. de Chim. et de Phys. t. lxiv. pp. 92–99.

for supposing that the germs and sporules of the monads would be more fortunate in surviving such an ordeal *.

I have as yet only had time to commence the study of the amount of heat which the Bacilli "spores" will resist as compared with the organisms from which they are derived. I began by endeavouring to ascertain whether these bodies were or were not capable of resisting a brief immersion in boiling water.

Dealing first with the Bacilli of urine, I procured a good supply of spores by inoculating an almost neutral specimen of boiled urine, contained in a flask plugged with cotton-wool, with another specimen of urine already swarming with such organisms, and placing the mixture in an incubator at 38° C.† In the course of two or three days a scum formed, in the threads of which spores were abundantly developed, and the fibres themselves during these and the two or three subsequent days broke up very extensively. This liquid (A), thoroughly well shaken, gave me a fluid teeming with Bacilli spores. Another liquid (B) was prepared by causing neutral urine to ferment at a temperature of 122° F. in an airless vessel. In this fluid, whilst the Bacilli themselves were swarming, their germs or spores were absent.

A number of bulb-tubes were then taken, and each of them was charged with one ounce of a urine whose acidity was equivalent to from 12 to 15 minims of liquor potassæ per ounce. Such a fluid is one which can be easily and certainly sterilized by heat, and this, of course, is an essential property for any nourishing liquid which is to be used in experiments as to the death-point of organisms. The ultimate object of such experiments being to enable us to decide not only as to the conditions under which a retention of life is possible in certain organisms, but to supply evidence bearing upon the possibility of a generation *de novo*, it seems absurd that an investigator should think of using in these death-point experiments a fluid possessing doubtful qualities—that is, one which, whilst it is known to be a nourishing fluid, may also be something more, a generating fluid. Yet it seems to me that Prof. Cohn's

* The observations of Pasteur, indeed, as well as of Tarnowski, tend strongly to show that the *spores* of the lower fungi generally are killed in fluids by a brief exposure to 60° C. (140° F.).

† At this temperature the boiled neutral fluid might not have fermented for many days; hence the fluid was inoculated in order to shorten the process.

and Dr. Eidam's experiments with Bacillus are to some extent open to this objection.

In acid urine of the kind mentioned we have a nourishing medium which, after ten minutes' boiling, is certainly not a generating medium. If, therefore, we charge a number of vessels with some of this liquid, to which fluid A has been added* in the proportion of one minim to the ounce, and another series with some of the same liquid to which fluid B has been added in similar proportions; and if we subsequently heat these mixtures to a similar extent, we shall be able to test the power of resistance to heat pertaining to the spores in liquid A compared with that pertaining to the mere rods and filaments in fluid B.

This I carried out in the following manner:—The necks of the bulb-tubes were drawn out in the blowpipe-flame, and the fluid within each was boiled over the flame for about a minute. The neck of the tube was then hermetically sealed, after which it was plunged into a vessel of boiling water, where it was allowed to remain for exactly ten minutes †.

The fluids of the two series similarly heated were then placed side by side in the incubator at 122° F. (50° C.), and the result in 25 trials (19 containing fluid A, and 6 containing fluid B) has been that not one of either series has fermented, though the tubes were kept from ten to fourteen days in the incubator. Yet in control experiments with the same urine boiled for ten minutes in plugged flasks and subsequently inoculated with an unheated drop of fluid A and of fluid B, fully developed fermentation was invariably set up in from sixteen to twenty hours—showing clearly that there was nothing in the nature of the fluid to impede the development of the organisms.

Having ascertained that hay-Bacilli also increased with about equal readiness in such acid urine, I have since executed a third series of experiments in which the inoculating material was similar to that of A in the fact that it swarmed with Bacillus spores, only it was composed of hay-infusion instead of urine, in which the organisms had gone on to spore-formation. The results were, however, in no way different. Out of 24 trials, fermentation did

* From a burette-tube kept for such fluids.

† The boiling in the can was adopted because the heat in this way is more constant and not subject to the continuance of those elevations which are so liable to occur in boiling over the flame (see p. 23).

not take place in a single instance, the inoculated mixture having been boiled as before for ten minutes.

Further than this I have not gone, at present, though it will be easy, when time permits, to ascertain the exact death-point of the Bacillus-spores, and whether their power of resistance is at all greater than that of the rods and filaments from which they are derived. Had it been found in the foregoing experiments that fermentation invariably occurred in the fluids in which the spores were contained and not in those holding the rods and filaments, we should have had a fair presumption that the spores had survived—since, in face of the possibility of the existence of a chemical ferment in the materials which served as inoculating agents, this would probably have been similar in both. Still this view could not have been certainly held; the differences in the medium which had led to spore-formation in the one and not in the other liquid *might* also have entailed a difference in their chemical products; so that, in the face of affirmative results, the possible influence of the medium and its chemical principles must have been differentiated from the influence of the organism alone.

It remained only to ascertain by similarly exact experiments whether any evidence could be obtained in favour of the statement that a previous desiccation enabled spores, in such a state, and when surrounded by thin albuminoid or gelatinous envelopes, to resist for a long time the moistening influence of water, and thereby to withstand for prolonged periods a degree of heat which would otherwise have proved destructive.

To test this point I proceeded in the following manner. I took a hay-infusion on which there was a well-formed scum containing myriads of the most typical spore-bearing fibres, partly entire and partly breaking up. This was put into a corked vessel and shaken vigorously for a few minutes, so as to procure a uniform dissemination of the spores through the liquid. Some of the thick, muddy-looking fluid was then poured upon an ordinary, clean, microscope slip, so as to cover it with a stratum of fluid, which was subsequently allowed to evaporate. In the course of three or four hours, when a dry opaque layer had been left upon the glass, the slip was placed in the dry chamber of an incubator at a temperature of 122° F., where it was kept exactly four days. The dry layer was then scraped off with a clean knife into a clean watch-glass, and to the resulting powdery material about thirty or forty drops of distilled water were added.

After allowing the powder to remain thus immersed for four hours, so as to imitate the stage of preparation of a hay-infusion, some of the stirred-up mixture was added to a quantity of urine having an acidity equivalent to eleven minims of liquor potassæ per ounce (about $2\frac{1}{2}$ per cent.) and a specific gravity of 1023. The addition was made in the proportion of two minims of the spore-containing liquid to each ounce of the urine, and with this well-shaken mixture nine bulb-tubes were charged. After their necks had been drawn out, the fluid in each of them was boiled over the flame for rather less than one minute, when the vessel was hermetically sealed. An interval of one minute having been allowed to elapse, each closed vessel was inverted and plunged into a vessel of boiling water for twenty minutes. Subsequently all were placed together in an incubator at a temperature of 122° F., and with them a control experiment in which some of the same urine had been boiled alone for twenty minutes in a small flask plugged with cotton-wool, and to which some drops of the original spore-containing mixture (not previously dried or heated) were added, in the above-mentioned proportion, when the urine was cool. This latter operation was effected by removing the cotton-wool plug for an instant, allowing the spore-containing fluid to drop into the urine, and then carefully replacing the plug, after the manner so often adopted by Professor Lister*.

The result of these experiments was as follows:—In sixteen hours the fluid in the control experiment was notably turbid, and a thin scum had formed on the surface at the expiration of 24 hours. The other nine fluids all remained quite clear, and showed no signs of turbidity during the ten days that they were retained in the incubator.

It did not seem necessary to go any further for the present, and neither did time permit of it. Enough had been done to show how little exact experiment would give any countenance to the hypotheses and wild assumptions which have of late been so rife in regard to the powers of endurance of Bacilli-spores—hypotheses and assumptions which seemed to their authors necessary, in face of the now-admitted fact that a hay-infusion will often ferment after it has been boiled even for several hours.

* Quart. Journ. of Microsc. Science, 1873, p. 384.

IX. *Bearing of the Experimental Evidence upon the Germ Theory of Disease.*

Though it may be conceded that with our present state of knowledge an affirmative decision in regard to the absolute proof of the present occurrence of Archebiosis may be still withheld, there is, I think, no similar warrant for suspense of judgment in regard to the Germ Theory of Disease or, as it is also called, the doctrine of Contagium Vivum. Existing evidence seems to me abundantly sufficient for the rejection of this doctrine as untrue*.

My urine and potash experiments will go far to illustrate this difference in the weight of the evidence in regard to the two questions.

A "sterilized" fluid—that is, one which left to itself would always remain pure—may be caused to ferment by the addition of a certain proportion of liquor potassæ devoid of all living things, especially if the influence of the potash be favoured by certain accessory physical conditions. This fact is admitted by M. Pasteur himself†. During the fermentation thus initiated, a matter (ferment) appears and increases, which is capable of spreading a similar process far and wide in suitable media.

But, on the strength of the analogy upon which the germ-theorists rely, we may find in such an experiment a warrant for the belief that in a healthy person, free from the contagium of typhoid fever or any other of its class, certain kinds of ingesta (solids or fluids), wholly free from all specific poison may,

* Since this paper was read, the doctrine has again been proclaimed—and never with more force and ability—by Dr. William Roberts (Brit. Med. Journal, Aug. 11, 1877). Its essential points may be stated in the words of its latest exponent. He says:—"I have already directed your attention to the analogy between the action of an organised ferment and a contagious fever. The analogy is probably real, in so far, at least, that it leads us to the inference that contagium, like a ferment, is something that is alive. . . . If, then, the doctrine of a contagium vivum be true, we are almost forced to the conclusion that contagium consists (at least in the immense majority of cases) of an independent organism or parasite; and it is in this sense alone that I shall consider the doctrine, ... it is more than probable, looking to the general analogy between them, that all infective diseases conform in some fashion to one fundamental type. If septic Bacteria are the cause of septicæmia, if the Spirilla are the cause of relapsing fever, if the *Bacillus anthracis* is the cause of splenic fever, the inference is almost irresistible that other analogous organisms are the cause of other infective inflammations and of other specific fevers."—*Sept.* 1877.

† See p. 31, note †.

with or without the favouring influence of other altered conditions, give rise to an independent zymotic process. And during the process thus initiated, a matter (contagium) appears and increases in certain of the fluids or tissues of the body, which is capable of spreading a similar disease far and wide amongst receptive members of the community.

Can the germless liquor potassæ plus the favouring conditions (the principal of which is a certain high temperature) be regarded as the "cause" of the fermentation? The answer does not admit of doubt: the effect in question would not have taken place without their influence. The old logical formula in regard to the word, *cessante causâ cessat et effectus*, completely justifies this point of view; and so also does the definition of Sir John Herschel. A "cause," said this philosopher, is "an assemblage of phenomena which occurring, *some other* phenomenon invariably commences or has its origin."

But there is a point of view which must not be lost sight of. It is of considerable importance, and has of late been dwelt upon by G. H. Lewes with his usual force and clearness. He says* :— "The fact that it is a convenience to select some one element out of the group, either for its conspicuousness, its novelty, or its interest, and that we call it the cause of the change, throwing all the other elements into the background of *conditions*, must not make us overlook the fact that this cause—this selected condition —is only effective in coalescence with the others. Every condition is causal; the effect is but the sum of the conditions."

This brings us to the only point of doubt which can possibly exist in regard to the interpretation of my experiment†. It is whether our most prominent causal element, the liquor potassæ, exercises its influence (*a*) partly upon the fluid and partly upon certain otherwise dead or impotent germs still lurking within the vessel, or (*b*) simply upon the mere chemical constituents of the fluid medium, but in such a way as actually to engender minute particles of living matter which thereafter appear as ferment-organisms.

If a practically dead germ can by any treatment be revived, it may take its place as one of the causal conditions leading to fermentation; hence it is that a certain reserve may still be maintained as regards the absolute proof of the possibility of a germless origin of common fermentations, and the almost simultaneous occurrence of a new birth of living units (Archebiosis).

* 'Problems of Life and Mind,' vol. ii. p. 390. † See p. 47.

But all similar grounds for reserve are absent—are non-existent, in fact—in regard to the bearing of this experiment upon the possibility of an occasional independent origin for zymotic disease, whether or not such disease is characterized by the appearance within the body of any distinctive living organisms*.

This I will now endeavour to demonstrate.

It is the process of fermentation which is supposed to be in part analogous to the zymotic disease. It is true that a contagious something becomes engendered during fermentation and during zymosis, by means of which the process or the disease may be spread abroad. But there are important differences in regard to the possible independent origin of the two processes which have hitherto been only too much neglected. The treatment of this subject has often been much too superficial. In order to produce a kind of pictorial effect which may easily captivate the imagination, difficulties are often ignored, and many new, modifying, or antagonistic points of view have even of late been treated as though they were non-existent.

A few words will suffice to make plain some of the differences between the respective conditions which would be operative in the germless origin of fermentation on the one hand, and in the *de novo* origin of a contagious disease on the other. And in so doing I shall be able, I think, at the same time, to show how much simpler it would be to bring about an independent zymosis than an independent fermentation—that is, if we are to rely on the analogy upon which the germ-theorists base their arguments.

During the great majority of fermentations living organisms make their appearance and rapidly multiply. These living organisms have been proved to be common producers of chemical principles, some of which are soluble ferments, others (like pyrogen) are poisons which may be almost as deadly as that of a serpent, whilst others still are inert and appear as mere pigment-granules. It is proved that some of these chemical principles act as true ferments †. It is thought, and it is probable, that the organisms themselves—altogether apart from their media and what else they may contain—may be capable of doing the same. Still this has not yet been definitely proved; so that the action of soluble

* The rule is, that organisms are present in fermentations, whilst they are, so far as we know, quite exceptional in zymotic diseases.

† Pasteur, 'Compt. Rend.' July 3, 1876, p. 4.

chemical ferments is at present almost better substantiated than that of the living organisms by which they may have been formed. By means of boiling alcohol and other agents these bodies can be isolated and freed from living impurity. It is, however, much more difficult entirely to separate minute living organisms from their media *, and consequently more difficult to be perfectly certain in regard to their potencies. It is, however, on account of the derivation of the chemical ferments from the living units, and because of the presence of these latter bodies in all fermenting mixtures, that their own agency is still regarded by many as essential to the initiation of ordinary fermentations. But, as I have already indicated, we much need further information as to the precise mode in which fermentation is initiated and carried on by soluble ferments like that which M. Musculus discovered in and separated from urine. If they (all or any of them) are capable of setting up fermentations in germless fluids in the course of which organisms appear, such phenomena would most effectually disprove an exclusive germ theory.

Turning now to the process of zymosis, we find the available generative conditions altogether different. Here we have to do not with fluids only, but with tissues and organs composed of living elements characterized by all kinds and degrees of activity. Some of them produce the various soluble ferments of the body, some may produce poisons, and others habitually lead to the formation of pigment-granules—vital acts severally similar in kind to those which the common ferment-organisms are known to manifest. Tissue-elements without number having such and multitudes of other properties are therefore ever present, capable under certain influences of being more or less easily diverted into unhealthy modes of action, so that many of them may become true living ferments in the modern sense of that term †, and therefore possible producers of chemical ferments (contagia) capable of

* The more efficient means of filtering organisms from their media, which we now possess, by means of porous earthenware, ought to be useful in this direction. Such organisms and their germs might be subsequently washed with several distilled waters, just as a chemist would wash a delicate precipitate. It would be strange, indeed, if this very mild usage interfered with the properties of organisms which at other times are credited with such remarkable powers of endurance.

† How legitimate this statement is may be seen from what M. Pasteur himself says. These are his most mature views :—"I have been gradually led to look upon fermentation as a necessary consequence of the manifestation of life, when that life takes place without the direct combustion due to free oxygen. . . . We

initiating some or the whole of the series of changes by which they were themselves produced, in other suitable sites.

The essential difference between the two problems thus becomes plain. The only point which my experiment leaves in the least doubtful in regard to the causal conditions initiating fermentation is, whether any latent, powerless, and, as it were, dead organized ferment may still, in spite of the usual evidence to the contrary, lurk in the seemingly "sterilized" fluid. This, however, is the very point about which there is no shadow of doubt in regard to zymosis. Possible ferments without number are, by necessity, present in the form of tissue-elements. So that if we are to be guided by the analogy upon which all germ-theorists so strongly rely, the independent generation of a zymotic process should, for the reason above specified, be incomparably more easy to be brought about than fermentation in a germless fluid. In regard to the independent origin of a zymosis, the all-important point is, not whether latent ferments exist, but whether any causes, or sets of unhygienic conditions, can rouse or modify, in certain special modes, the activity of any of these myriads of potential ferments of which the human organism is so largely composed. And if, as some germ-theorists would have us believe, impotent germs of common ferment-organisms, incapable of exclusion, are also widely disseminated throughout the body, these, if they are such unavoidable elements, could (in regard to the ætiology of disease) only be looked upon as components of the body, ranking side by side with the tissue-elements themselves.

Thus such organized ferments or germs as are possibly absent from the "sterilized" experimental fluids are confessedly present by myriads in persons who may be sickening under the influence of various unhygienic conditions or non-specific states of the system; and the only point which is regarded as doubtful in connexion with the *de novo* origin of a zymosis is what analogy might lead us to affirm as completely proved by my experiments, viz., that certain conditions, or states of system, may be capable of rousing some

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may partially see, as a consequence of this theory, that every being, every organ, every cell which lives or continues its life without making use of atmospheric air, or which uses it in a manner insufficient for the whole of the phenomena of its own nutrition, must possess the characteristics of a ferment with regard to the substance which is the source of its total or complementary heat."—*Compt. Rend.* 1872, t. lxxv. p. 784.

of such ferments into a specific kind of activity, wholly apart from the influence of any specific contagia coming from without*.

Even if independent ferment-organisms of common or special kinds do make their appearance during any process of zymosis originated in the manner above suggested, they would, from the point of view of the ætiology of disease, be just as much consequences of the morbific influences, as proliferation of tissue-elements is a consequence of the direct application of acetic acid or any other irritant.

But here, in order to make this point of view more plain, a short digression is necessary.

The intracellular fermentation in vegetal tissues supplies us with a kind of link between the ordinary processes of fermentation and the zymotic processes of animals. MM. Lechartier and Bellamy, as well as Pasteur and others, have now clearly shown that in vegetal tissues placed under certain abnormal or unhealthy conditions, fermentative phenomena take place essentially similar to those occurring in solutions containing independent ferment-organisms. And just as the vegetal cell can do what, in other

* Whilst the last sheets of this paper are passing through the press, a very interesting address by Dr. B. W. Richardson, F.R.S., has been published ('Nature,' Oct. 4, 1877), entitled "A Theory as to the Natural or Glandular Origin of the Contagious Diseases." In it the author advances many strong arguments against the germ theory; he also propounds some interesting speculations as to the mode of origin and action of the chemical principles, or poisons, which constitute, as he believes, the "contagia" of the communicable diseases. Some such views make a very fitting supplement to the doctrines which I have been here attempting to establish in regard to these diseases; only we must, as Dr. Richardson observes, seek gradually to put well-proven facts in the places now occupied by mere speculations. In regard to the practical aspects of the two opposite doctrines, Dr. Richardson makes some very pertinent observations. "If the contagium vivum view be true," he says, "if the air around us is charged with invisible germs, which come from whence we know not, which have unlimited power to fertilize, which need never cease to fertilize and multiply, what hope is there for the skill of man to overcome these hidden foes? Why on some occasion may not a plague spread over the whole world, and destroy its life universally? Whilst, on the other hand, if the opposite notion be true, we have complete mastery over the diffusion of the poisons of all the communicable diseases. We have but to keep steadily in view that the producing and the reproducing power is in the affected body, and we can, even with our present knowledge, all but completely limit the action to the propagating power of that body—its power, I mean, of secretion and diffusion of secretion."—*Oct.* 6, 1877.

cases, the independent organism does, so it is supposed that in the process of zymosis tissue-elements may take on a specifically faulty action, leading to the formation of certain chemical principles or "contagia" in the fluids or tissues of the animal body; so that, in the great majority of zymotic diseases, offcast particles from the body, whether living or dead, when saturated with such principles, may constitute the veritable contagia by which the specific disease is spread abroad amongst the community.

In the majority of the cases of intracellular fermentation no independent organisms are generated, though in others, as in that of the beetroot and the potato, they are invariable concomitants. Similarly in the majority of zymotic diseases no independent organisms are generated, though in others, such as relapsing fever and splenic fever, they are invariable concomitants; and being engendered in diseased parts and fluids they may thereafter themselves act either as real contagia or as carriers of contagion.

The causal conditions capable of inducing fermentation in the beetroot and the potato, and with it the appearance of Bacteria in swarms throughout their tissues, are known, and have no ordinary connexion with preexisting Bacteria. And similarly the causal conditions capable of inducing relapsing fever and splenic fever, though not so definitely known, may nevertheless have no ordinary connexion with preexisting Spirilla and Bacilli resembling those which appear in the blood or tissues of the patients suffering from either of these diseases.

Thus the mere fact that in certain zymotic diseases living organisms have been proved to appear, affords of itself no support whatever to an exclusive germ theory, as I shall, after this digression, endeavour to show.

The fact may be quite otherwise explained, either (1) in accordance with the views of certain germ-theorists, though these are in direct opposition to the statements of others of the same party; (2) in accordance with the statements of this second section of the germ-theorists, supplemented by a belief in heterogenesis.

(1) The presence of latent germs of common though modifiable ferment-organisms throughout the body is invoked by one section of the germ-theorists, who contend that certain altered states of health, together with altered vitality of tissues, may rouse such hitherto latent common organisms into activity, and occasionally convert them into so-called "specific" forms capable of new actions. But based as this view is upon wholly insufficient evidence, and

with its fundamental position denied by other leading germ-theorists, it would, even had it been securely founded, be quite inadequate to meet the necessities of their position. A special zymotic disease, which had arisen in the manner above indicated, would assuredly have had what is termed a *de novo* origin—it would have started from no specific cause, and would never have developed, but for the existence of those "determining conditions" which brought about the altered state of health and tissues. This group of conditions would therefore constitute the cause of the disease; and inasmuch as, by the hypothesis we are now considering, the common germs are held to be *ever present and unavoidable*, any changes or developments which they might take on could only be studied in the same rank and side by side with those of the other tissue-elements—that is, as consequences or phenomena of the disease.

(2) It was originally affirmed by Prof. Burdon Sanderson*, and it has of late been distinctly reasserted by M. Pasteur†, that the blood and internal tissues of healthy animals and of man are entirely free from ferment-organisms or their germs. Some have sought to modify this view, on the strength of certain experiments which are so extremely inconclusive as to make it almost puerile to have brought them forward‡.

For, however strong the evidence is that living units may, on certain occasions, be even proved experimentally to appear in fluids in which no living matter previously existed (archebiosis), it is even stronger to show that, under certain conditions, similar low, independent forms of life may originate in the midst of living tissues previously free from them, by a kind of transformation

* Thirteenth Report of the Medical Officer of the Privy Council.

† Comptes Rendus, April 30, 1877, p. 900.

‡ Cutting out portions of the internal organs of recently killed animals, enveloping them with superheated paraffine, and then placing them in an incubator at a suitable temperature to see whether germs and organisms will appear, would, even if taken alone, obviously permit no certain conclusion to be drawn from their appearance. But the evidence relied upon by Sanderson and Pasteur tends as strongly to show that they are not developments of preexisting germs, as certain other evidence subsequently to be mentioned tends to show that they are heterogenetic products (Trans. of Path. Soc. 1875, p. 267). Yet, following a now long-established custom of ignoring the possibility of the heterogenetic origin of Bacteria, the results of such experiments are by some supposed to demonstrate the existence of latent germs in an organ like the spleen, for instance, which is wholly cut off from outside communication—and even when the blood itself is declared to be germless.

(heterogenesis) of some of the units of protoplasm, which though still living, have been modified in nature and tendency by reason of their existence in a partially devitalized area.

The evidence in favour of this last kind of change may be regarded wholly apart from that furnished by the closed-flask experiments, from which it is quite distinct. It suffices, I think, to account for the presence of organisms in some of those local and general diseases with which they are known to be associated, and therefore to complete the proof that even such disease may originate *de novo* (as well as by contagion), and that the organisms which characterize them are, in such cases, consequences or concomitant products, not causes of the local or general conditions at whose bidding they appear. The elements of the proof are these :—

(*a*) First there is the evidence which has been adduced by various observers as a result of the study by the microscope of the mode in which organisms appear within tissue-elements. I do not lay much stress upon this here, because evidence of such a nature is more open to various objections than that which is to follow*.

(*b*) Although the blood and internal tissues of healthy animals and of man are free from independent organisms and their germs, yet such organisms will habitually show themselves after death, in the course of a few days, throughout all the organs of one of the lower animals or of man—even when life has been abruptly terminated during a state of health. It cannot be said, in explanation of this, that the organisms naturally present in the intestinal canal have been enabled to spread through the body so as to reach its inmost recesses *after death*—since many of the organisms found are motionless, and others have mere to-and-fro movements of a non-progressive character. The blood, again, has ceased to circulate, so that this fluid, germless during life, cannot after death be considered to act even as a carrier. If the organisms themselves cannot make their way through the tissues, and if no carrier exist, they must naturally have been born in or near the sites in which they are found.

Phenomena of this kind are to be witnessed even in insects, such as silkworms and flies; and the organisms that habitually develope in them after death are, as in the case of higher animals, just such organisms as appear in some of their best-known contagious diseases†. Certain of these diseases, like "muscardine,"

* On this subject see 'Beginnings of Life,' vol. ii. p. 342.

† Ibid. pp. 327, note 1, and 330, and Trans. of the Patholog. Soc. 1875, p. 343.

seem to be generable *de novo* at the will of the operator by merely placing the animal for a few days under particular sets of unhealthy conditions.

(*c*) Some of the ferment-organisms may also be made to appear at will in certain parts of still living and previously healthy animals by determining in any such part either (1) a greatly lowered vital activity, or (2) an active perversion of the nutritive life of the part of considerable intensity.

1. This subject has been studied experimentally by Messrs. Lewis and Cunningham *, two thoroughly competent and trustworthy observers, whose researches during recent years have won for them a deservedly high reputation. They say, "The object of the experiments was to ascertain whether, by interfering with the vascular supply of certain tissues and organs of the body of an animal without injuring the isolated tissue, we should be able within the course of some hours to detect organisms in those parts in the same manner as we had been able to do when an animal had been killed under chloroform and set aside in a warm place. We found that such was the result, and that a kidney, for example, when [its artery was] carefully ligatured without interfering with its position in the abdomen, would be found after some hours to contain precisely similar organisms; whereas the other kidney, whose circulation had not been interfered with, contained no trace of any vegetation whatever" †.

* 'The Fungus-Disease of India,' Calcutta, 1875, p. 89.

† On September 17, 1877, I had an opportunity of seeing how far this would hold good for the human subject. On that day I made an examination, 12 hours after death, of the body of a young man who had been suffering from severe heart-disease in University College Hospital. His temperature had only been slightly raised for about 48 hours before death; but there was reason for believing that embolic obstructions had recently occurred in one or both kidneys. Abundant "vegetations" were found on the mitral and aortic valves, and two or three embolic patches existed in each kidney, some being recent and others of older date. One large yellowish embolic patch was likewise found occupying the upper extremity of an enlarged spleen. Some blood from the right ventricle and some urine from the bladder, carefully removed with capillary tubes, on examination with the microscope and a $\frac{1}{12}$ object-glass, showed no organisms of any kind. Portions of tissue cut from the interior of the liver also showed no organisms. On the other hand, the embolic patch in the spleen as well as those in the kidney, both old and recent, showed, when portions of their disintegrated substance were examined, organisms, more or less abundantly distributed, similar to those which Messrs. Cunningham and Lewis have figured. Some were Bacilli and some were more like what Cohn now distin-

2. Facts of this second order have been thoroughly established by the important researches of Professor Burdon Sanderson. He says*:—"If a few drops of previously boiled and cooled dilute solution of ammonia are injected underneath the skin of a guinea-pig, a diffuse inflammation is produced, the exudation liquid of which is found after twenty-four hours to be charged with Bacteria." "Other chemical agents," he adds, "will lead to the same results, and always under conditions which preclude the possibility of the introduction of any infecting matter from without."

Elsewhere† the same investigator refers to experiments which were made about the same time in order to throw light upon the cause of the appearance of Bacteria in certain peritoneal exudations, and to ascertain whether or not their presence was to be considered as "a mere result of the intensity of the peritonitis." He says:—"To determine this experiments were made during the following month (May 1871), which consisted in inducing intense peritonitis by the injection, not of exudation liquids, but of chemical irritants, particularly dilute ammonia and concentrated solution of iodine in hydriodic acid. As regards the ammonia, precautions were taken to guard against contamination by boiling and cooling the liquids as well as the implements to be used immediately before injection. In the case of the iodine solution this was, of course, unnecessary. In every instance it was found that the exudation liquids, collected from twenty-four to forty-eight hours after injection, were charged with Bacteria, whence it appeared probable that the existence of these organisms was dependent, not on the nature of the exciting liquid by which the inflammation was induced, but on the intensity of the inflammation itself."

From the various evidence more or less fully referred to in the present section it seems to me legitimate to conclude:—

First, that if we are to be guided by the analogy now dwelt upon as existing between fermentation and zymosis, it would be per-

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guishes as Vibriones. They were not so abundant as to be always found without careful examination; and, on the other hand, in the diseased splenic tissue there were a multitude of small acicular crystals which an inexperienced observer might mistake for motionless organisms. In the lower healthy portion of the spleen no organisms were found.

* Transactions of the Patholog. Soc. 1872, p. 306–308.

† Reports of the Med. Officer of the Privy Council, &c., New S., No. vi., 1875, p. 57.

fectly certain that the latter process can originate *de novo*—that is, under the influence of certain general or special conditions, and where specific contagia of any kind are at first absent though they subsequently appear as results or concomitant products. So that an exclusive theory of "contagion," as the only present cause of communicable diseases, is not supported by experimental evidence.

Secondly, that some contagia are mere not-living chemical principles, though others may be living units.

Thirdly, that even in the latter case, if the primary contagious action be really due to the living units and not to the media in which they are found, such primary action is probably dependent rather upon the chemical changes or "contact actions" which they are capable of setting up than upon their mere growth and vegetative multiplication.

Fourthly, that where we have to do with a true living contagium (whether pus-corpuscle or ferment-organism), the primary changes which it incites are probably of a nature to engender (either in the fluids or from the tissue-elements of the part) bodies similar to itself, so that the infected part speedily swarms therewith. When pus from a certain focus of inflammation comes into contact with a healthy conjunctiva, and therein excites a contagious form of inflammation, no one adopts the absurd notion that all the pus-corpuscles in this second inflammatory focus are the lineal descendants of those which acted as the contagium; and the mode of action may be altogether similar when matter containing Bacilli, by coming into contact with a wounded surface, gives rise to splenic fever and the appearance of such organisms all through the body. The old notion about the excessive self-multiplication of the original contagium is probably altogether erroneous.

Thus all the distinctive positions of those who advocate a belief in the so-called "Germ-theory of Disease," or rely upon the exclusive doctrine of a "Contagium vivum," seem to be absolutely broken down and refuted. We may give that attention to the appearance and development of independent organisms in association with morbid processes which the importance of their presence demands, but we must regard them as concomitant products, and not at all, or except to an extremely limited extent, as causes of those local and general diseases with which they are inseparably linked.